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Regulatory Corporate Governance and the Valuation of IPO Firms

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Abstract

This study aims to evaluate the effect of regulatory corporate governance mandates on the valuation of equity-issuing firms in the U.S. Using a matched sample, we examine how the Exchange Listing Requirements, specifically, and the Sarbanes-Oxley Act (SOX), generally, affect IPO valuations. Board structure compliance provides no consistent valuation benefit. We find some evidence of negative effects for firms whose board structure is significantly altered by Reform and among small firms. The absence of increased valuations post-Reform suggests that there is little to offset the loss of private control benefits that Reform represents (post-Reform insider ownership and founder involvement are lower) and, thus, at the margin, Reform creates incentives for some firms to stay private. While the 2012 JOBS Act reduced the burden of registration, reporting and accounting requirements of SOX for small firms, it did nothing to change the board structure requirements of these firms. The results of this study together with those of Wintoki (2007) and Rhodes (2018) suggest that regulations pertaining to the board structure requirements of small equity-issuing firms should either be modified to allow more flexibility or repealed altogether. If lawmakers ultimately relax these requirements, future studies may focus on changes in board structures, private benefits of control, and the rates at which firms access public equity markets.

Keywords: *Initial Public Offerings, Corporate Governance, Regulation*

JEL classification*: G32; G34; G38*

Introduction

The U.S. financial and accounting scandals of the early years of the 21st century (e.g. Enron, WorldCom, HealthSouth) spurred an increased interest in corporate disclosure and reporting and in corporate governance structures. They raised the question of whether existing governance structures were

characterized by rent extraction rather than shareholder wealth maximization. The passage of the Sarbanes-Oxley Act of 2002 (SOX) represented, in part, an adoption of the rent extraction view and represents a significant departure from the traditional form of federal securities regulation. Historically, federal regulation had focused primarily on assuring the adequacy of corporate disclosures, rather than upon substantive corporate governance requirements (Romano, 2005). While a significant portion of SOX is concerned with disclosure and the role of auditing firms in certifying disclosures, among the governance requirements of SOX are provisions requiring independent audit committees, restrictions on the purchase of consulting services from auditors, prohibition of loans to officers, and executive certification of financial statements.

Even more extensive regulation of corporate governance structures arose from the calls for reform that led to SOX but are outside of its actual provisions. On February 13, 2002, before the final passage of SOX, the SEC called for the major stock exchanges to review their governance requirements (Chhaochharia and Grinstein, 2007). This led to the development of governance rules more extensive than those contained in SOX itself. Similar rules were submitted by the NYSE and NASDAQ during 2002 and approved by the SEC in November 2003. Among the governance provisions included in these exchange listing requirements (ELR) were:

1. All firms must have a majority of independent directors.
2. The requirements for independence were made more rigorous than previous definitions.
3. The compensation, nominating, and audit committees of the board of directors must consist of independent directors.
4. All audit committee members must be financially literate and at least one member must have accounting or related financial management expertise.
5. In addition to its regular sessions, the board should hold additional sessions without management.

Early literature suggested a relation between board structure and firm value with smaller (Jensen, 1993; Yermack, 1996) and more independent boards (Baysinger and Butler, 1985) contributing to firm value. However, as Bhagat and Black's (1999) review of the early board structure literature suggests, these were never unambiguous conclusions and are, arguably, contrary to the weight of the evidence. Nonetheless, SOX and the ELR (henceforth collectively referred to as Reform) effectively codified elements of this view by requiring certain minimum levels of board independence for publicly traded firms (See SOX subsection 301, NASD Rule 4350(c), and NYSE Rule 303A).

While the relationship between corporate governance and firm valuation has been a topic of interest in the recent financial literature (Gompers, Ishii and Metrick, 2003; Cremers and Nair, 2005; Bebchuk and Cohen, 2005; Brown and Caylor, 2006; Bebchuk, Cohen and Ferrell, 2009), as has interest in the general corporate governance effects of the SOX (Jain and Rezaee, 2006; Chhaochharia and Grinstein, 2007; Litvak, 2007; Wintoki, 2007; Zhang, 2007; Li, Pincus, and Rego, 2008; Banerjee, Humphrey-Jenner and Nanda, 2015; Bradley and Chen, 2015; Guo and Masulis, 2015), and its influence on the going private/dark decision (Engel, Hayes, and Wang, 2007; Marosi and Massoud, 2007; Leuz, Triantis, and Wang, 2008; Li, 2014) there has been little examination of this in the context of equity-issuing firms. Our objective is to fill this gap in the literature by focusing on the effects of the corporate governance reforms on the valuation of equity-issuing firms at the IPO.

We are particularly interested in examining the impact of these changes on firm valuations, as opposed to IPO initial returns or pricing (e.g., Johnston and Madura, 2009). While initial returns (or underpricing) reflect the reward to investors associated with participating in an IPO and “money left on the table” by issuers and underwriters, valuations instead reflect the direct impact of regulatory corporate governance reforms on overall shareholder wealth. Because issuers often ultimately benefit from such intentional underpricing, it is sometimes ambiguous whether, overall, shareholders are better or worse off when underpricing changes as a result of a regulatory change like SOX.[1] On the other hand, because valuations are direct determinants of

[1] There is empirical evidence that, while underpricing suggests IPOs are undervalued by underwriters relative to their aftermarket valuations, IPOs are actually overvalued by underwriters relative to their non-equity issuing peers (Purnanandam and Swaminathan, 2004). So, underpricing is not a reliable indicator of IPO valuations. IPO valuation is addressed only rarely

shareholder wealth, shareholders are clearly harmed if valuations fall or clearly benefit if valuations rise as a result of a regulatory change. Thus, by examining valuations as opposed to initial returns, we hope to obtain a clearer picture of the impact of regulatory corporate governance reforms on IPO firms and their owners.

This study is motivated by the existing theoretical and empirical literature, discussed in the next section, that suggests that Reform may have moved the typical IPO firm away from its optimal board structure and that the costs of compliance may deter issuers, particularly small firms, from accessing public equity markets. In addition, firms that choose to go public are forced to surrender some private benefits of control in order to comply with the requirements of Reform.

Similar to Banerjee, Humphrey-Jenner and Nanda (2015), Guo and Masulis (2015) and Bradley and Chen (2015), we utilize Reform as a natural experiment to test SOX and the ELR's effect on IPO valuations. We take 5 years of post-Reform IPO data from 2005-2009 and 6 years of pre-Reform IPO data from 1996-2001. We examine these samples on several summary statistics, documenting that the pre-Reform and post-Reform IPO markets are different in important ways, which suggests that a self-selection effect is occurring post-Reform. We confirm that the bubble years of 1999-2000 are not the cause of these differences. To control for potential self-selection effects, we then construct a matching sample for our post-Reform subsample of IPOs from the pre-Reform subsample of IPOs using propensity score matching, where we use firm characteristics and market sentiment measures as our matching criteria. We then use a new methodology to form relative valuation measures founded upon several commonly used accounting-based valuation ratios (e.g., price-to-earnings and market-to-book ratios). This methodology controls for both industry effects and the negative ratio problem, allowing a far more complete sample than is usually possible in IPO valuation studies.

We find that for a subsample of firms where the deviation of actual board structure from predicted board structure is greater, there is some evidence of a negative effect from board structure compliance. These results are generally consistent with the theoretical and empirical "one size does not fit all" board structure literature (Hermalin and Weisbach, 1998; Raheja, 2005; Adams and Ferreira, 2007; Harris and Raviv, 2008; Boone, Field, Karpoff, and Reheja, 2007; Coles, Daniel, and Naveen, 2008; and Linck, Netter, and Yang, 2008). We also find that, for some valuation measures, a general Post-Reform dummy, reflecting the effects of the remaining governance and the disclosure provisions of SOX, is negative and significant, suggesting Reform has actually decreased IPO valuations.[2] The effect is stronger for firms whose actual board structure differs from predicted structure more. The only evidence that Reform has increased IPO valuations is limited to small firms not subject to SOX Section 404 whose board structures were more significantly altered by Reform, suggesting firms exempt from this costly provision may have fared better than the general population of IPO firms. We also document that the ownership structure of firms is significantly different for firms going public post-Reform, with post-Reform firms having substantially lower levels of insider ownership and board presence and decreased founder involvement (i.e. firms with high private benefits of control are less likely to go public post-Reform). Thus, our overall conclusion is that the absence of increased valuations post-Reform suggests that there is little to offset the loss of private control benefits that Reform represents and thus that, at the margin, Reform creates incentives for some firms to stay private.

The next section presents the literature review and our primary hypotheses. The following section describes the data and its sources. The fourth section documents the differences in our pre-Reform and post-Reform samples, which suggest that Reform has affected the population of IPO firms. The fifth describes the methodology and the model for our primary analysis. The following section presents the primary results and robustness tests and the last section concludes.

Literature Review

in the literature, presumably because of the difficulty of data issues (see Kim and Ritter, 1999; Purnanandam and Swaminathan, 2004, and the discussion below).

[2] Our results compliment those produced by Larcker, Ormazabal and Taylor (2011), who examine the market reaction to 18 changes to corporate governance regulation related to CEO pay, proxy access, and CEO-chairman duality. Consistent with the notion that corporate governance practices are set so as to maximize shareholder wealth, they produce evidence of a negative stock price reaction for firms whose governance practices would be most significantly altered by the proposed regulations.

The theoretical (e.g. Hermalin and Weisbach, 1998; Raheja, 2005; Adams and Ferreira, 2007; and Harris and Raviv, 2008) and empirical (e.g. Boone, Field, Karpoff, and Reheja, 2007; Coles, Daniel, and Naveen, 2008; and Linck, Netter, and Yang, 2008) literatures on optimal board structure have continued to expand with almost all reaching the conclusion that when it comes to board configuration "one size does not fit all." For example, young small high-growth firms with high monitoring costs might do better with a smaller more insider-oriented board, while old large slow-growth firms with low monitoring costs might do better with a larger outsider-dominated board.[3] Thus, the literature has reached a point 180 degrees from the regulatory approach taken in Reform, which requires all publicly-listed firms, regardless of firm size, to structure their boards in the same way. Compliance may also be costly in monetary terms, particularly for smaller firms. Linck, Netter and Yang (2009) find that post-SOX there are significant increases in director pay and overall director costs, especially among smaller firms.

The literature suggests that Reform may have led the typical IPO firm (a young small growth firm) away from its optimal board structure. If so, then Reform presents a form of natural experiment where post-Reform firms, whose boards may be constrained by SOX and the ELR's "one size fits all" approach, can be compared with matched pre-Reform firms, which were free to organize their board structure as they saw fit. This provides an opportunity to test the relationship between board structure and firm value without the usual constraints of the endogeneity problem. If Reform moves board structure away from an unconstrained optimum, and board structure affects value, IPO valuations should be lower compared to pre-Reform matched firms. If Reform moves board structures away from a rent extraction configuration, then IPO valuations should be higher compared to pre-Reform matched firms. Thus, our first hypothesis is:

HYPOTHESIS 1:

H_0: The SOX and ELR board structure requirements have had no effect on IPO valuations.
H_{1A}: The SOX and ELR board structure requirements have had a positive effect on IPO valuations.
H_{1B}: The SOX and ELR board structure requirements have had a negative effect on IPO valuations (one size does not fit all).

We test hypothesis one by regressing IPO valuations on a Compliance Index variable that measures the extent to which IPO firms comply with the board structure requirements of Reform at the time of offering. A positive relationship between valuation and the Compliance Index supports H1A, suggesting that Reform moved IPO firms' board structures away from a rent extraction configuration toward a structure that maximizes shareholder wealth. On the other hand, a negative relationship between the Compliance Index and firm valuation supports H1B, consistent with the theoretical and empirical research that argues that one size does not fit all when it comes to optimal board structure, and that constraining IPO firms' board structures in this way reduces shareholder wealth. Finally, an insignificant relationship between the Compliance Index and firm valuation supports the null hypothesis, consistent with previous studies that have found that once the endogeneity of board structure is controlled for, that board structure is not consistently related to firm value (Agrawal and Knoeber, 1996; Lehn, Patro, and Zhao, 2009).

Reform also produces other valuation effects that must be considered. SOX, particularly Section 404, also imposes significant monetary costs on listed firms. Morgenstern and Nealis (2004) argue that disclosure requirements resulting from SOX have significantly increased the costs of being a publicly listed company and disproportionately so for smaller firms because compliance is largely the same for all firms, regardless of size, and involves significant fixed costs. Iliev (2010), examining a sample of firms with public floats between $50-100 million, finds the marginal cost of compliance with SOX to be high. The marginal increase in audit fees for firms that comply is over $698 thousand higher than noncompliant firms. He also estimates total compliance costs (including outside vendor, internal labor, and internal non-labor costs in addition to audit fees) using proportions found in a FEI (2005) survey to be $2.3 million per year, so that indirect costs are slightly more than twice the audit costs.

[3] Wintoki (2007) has documented that there is significant cross-sectional variation in the wealth effects around the dates of SOX and the ELR with older larger firms fairing better and firms with more growth opportunities and less certain operating environments doing worse in the general population of firms.

Reform also introduced a substantial change in the value of private control benefits between non-SEC-registered unlisted firms and SEC-registered listed firms. Post-Reform, insiders wishing to list on a stock exchange (SEC registration is required for listed firms) must assemble a board of directors that is dominated by outsiders. Or, in other words, insiders must be willing to surrender a significant portion of the private benefits of control in order to be a publicly traded exchange-listed firm. Such considerations may affect public valuations as well, if they represent agency costs, but some may not. Doidge, Karolyi, Lins, Miller, and Stulz (2009) document that when private benefits of control are high, firms are less likely to cross-list in the U.S. because of the constraints on the consumption of private benefits that result from such listings, suggesting these concerns do indeed influence corporate behavior. Thus, Reform has imposed significant monetary costs and reduced control benefits of insiders in publicly listed firms.

The evidence increases that Reform has influenced corporate behavior in the general population of firms. Block (2004), Engel, Hayes, and Wang (2007), Marosi and Massoud (2007), Leuz, Triantis, and Wang (2008), and Li (2014) all find that firms are more likely to go private or go dark (i.e. delist and deregister with the SEC) after the passage of SOX and that the characteristics of firms that do so are related to the costs that SOX imposes. If Reform has made it more likely for a public firm to go private or, more commonly, go dark, that raises the question whether Reform has also made it more likely for a private firm to stay private or stay dark.

Insiders would choose to go public if the firm's value if public exceeds its value if private, including the value of private control benefits. Reform changes this calculus in several ways. It may cause the firm's governance structure to deviate from its optimum. It increases disclosure costs, significantly so for small firms. It reduces the private benefits of control that remain when the firm goes public. All these factors reduce the incentive to take a private firm public. Coates (2007) argues that the benefit of SOX is that public companies will face a lower cost of capital as investors face a lower risk of fraud and theft losses and benefit from more reliable financial reporting, greater transparency, and greater accountability. A lower cost of capital would imply that the valuation of the firm will be greater if expected cash flows are unchanged. The cash flow effects of regulation-imposed corporate governance structures depend upon whether the original structures served to facilitate rent extraction or maximized shareholder value. Increased disclosure costs and a less than optimal regulation-imposed governance structure mean that expected cash flows will fall for public companies. Of course, the one size fits all structure of regulatory corporate governance might be an improved governance structure, which could increase expected cash flows. Thus, whether the public valuation of a firm rises or falls would indicate whether the benefits of a lower cost of capital offset the cash flow effects of compliance and regulatory corporate governance.

Private control benefits are not necessarily a part of this calculus, however. It depends upon the extent to which they represent agency costs that are included in valuation. Some private benefits of control are valuable to insiders, but not necessarily costly to shareholders. To offset the decline in these types of private control benefits for firms going public, it would be necessary for public valuations to rise post-Reform for the effect of Reform on the going public/staying private decision to be neutral. That is, for Reform not to deter IPOs at the margin, valuations must be higher post-Reform to offset the loss of control benefits that do not represent agency costs.

Since we are also interested testing the effect of these other elements of Reform on the valuation of IPO firms, our second hypothesis is:

HYPOTHESIS 2:

H_0: Reform, in general, has had no effect on IPO valuations.
H_{2A}: Reform, in general, has had a positive effect on IPO valuations.
H_{2B}: Reform, in general, has had a negative effect on IPO valuations.

To test hypothesis two, we form a Post-Reform dummy that equals one for firms that go public post-Reform and zero for those firms that go public pre-Reform. If there is a positive relationship between the Post-Reform dummy and firm valuation, that supports the idea that Reform has increased firm valuations by reducing agency costs and thereby reducing firms' costs of capital (Coates, 2007). A negative relationship

between the Post-Reform variable and valuation suggests that the costs of compliance and/or the impact on the private benefits of control outweigh any positive effects on the cost of capital. An insignificant relationship between the Post-Reform dummy and valuation suggests that either Reform, in general, has not impacted firm valuations or that the positive effects on cost of capital just offset the negative impact associated with the cost of compliance and/or private benefits of control.

Research and Methodology

Data

Our initial dataset consists of two samples of IPOs, one prior to and another following the implementation of Reform, identified using the Thomson Financial Securities Data Company database (SDC). The pre-Reform sample is drawn from all U.S. issues between 1996 and 2001. We select 1996 as the lower bound on the pre-Reform date range because data necessary to this study must be collected from IPO prospectuses, which became available on the SEC's EDGAR online database beginning with IPOs issued in 1996 (www.sec.gov/edgar). The post-Reform sample covers all U.S. IPOs issued between 2005 and 2009. The post-Reform sample begins in 2005 because, though SOX was passed in 2002, most of the final compliance deadlines for the major regulations resulting from SOX and the ELR were in October 2004. As is standard in the IPO literature, we exclude regulated utilities and financial firms (SIC codes 4900-4999 and 6000-6999), unit offerings, American Depository Receipts, limited partnerships, IPOs with offer prices less than $5, and best efforts offerings. Applying these filters yields a post-Reform sample of 380 IPOs and a pre-Reform sample of 1588 IPOs.

From SDC we also collect information on offering characteristics, including the offering date, offer price, number of primary, secondary and total shares offered, proceeds, listing exchange, and the identity of the lead underwriter. We obtain the first closing market price, number of shares outstanding after the offering and market return data from the Center for Research in Security Prices (CRSP) database. Consumer Price Index (CPI) information is extracted from the St. Louis Federal Reserve's website.[4] Balance sheet and income statement data after the offering as well as SIC codes are collected from Compustat. Firm founding dates, used to compute firm ages, are obtained from Jay Ritter's website along with information on underwriter rankings as defined in Loughran and Ritter (2004).[5] We compute valuation variables for other firms in each IPO's industry using price information from CRSP and balance sheet or income statement data from Compustat.

The final matched sample requires a significant amount of hand-collected information from prospectuses made available on the SEC's EDGAR online database. From EDGAR, we collect information on shares outstanding prior to the offering as well as pre-offering balance sheet and income statement data because it is either unavailable or unreliable in SDC. To this information we add detailed data on ownership and board structure immediately after the offering, including the extent to which firms comply with the board independence requirements implemented by the exchanges following the passage of SOX. From prospectuses we also retrieve information on company founders, the use of the offering proceeds, and whether the firm has a dual-class common share structure.

Comparing the Pre- and Post-Reform IPO Markets

A number of authors have documented the decrease in the number of IPOs, and particularly small IPOs, in the United States following the end of the Internet IPO bubble (Zingales, 2007; Weild and Kim, 2009; Asquith and Rock, 2011; Doidge, Karolyi, and Stulz, 2013; Gao, Ritter, and Zhu, 2013). Our data confirm this pattern.

The statistics in Table 1 report differences in the means of IPO characteristics between pre- and post-Reform firms over our sample period (1996-2001 and 2005-2009). Panel A reports differences including the bubble period (1999-2000) in the pre-Reform period and results in Panel B exclude the bubble period. Proceeds and

[4] http://research.stlouisfed.org/fred2

[5] https://site.warrington.ufl.edu/ritter/ipo-data/

market capitalization are adjusted for inflation and reported in 2009 millions of dollars. Panel A shows that the post-Reform sample is composed of larger, older, more leveraged firms raising more

Table 1: Descriptive Statistics: Full Sample of Pre-Reform Offerings (1996-2001) versus Post-Reform Offerings (2005-2009)

This table provides descriptive statistics (i.e. means) for the full sample of IPOs from 1996-2001 (Pre-Reform) and 2005-2009 (Post-Reform) periods for a number of IPO characteristics. Panel A includes the bubble period (1999-2000), while Panel B excludes it. Proceeds and market capitalization are in inflation-adjusted 2009 millions of dollars. The table also tests for the equality of the sub-sample means. ***,**, and * indicate significant differences at the 1%, 5%, and 10% levels, respectively.

Differences in Averages

Panel A: Pre-Reform Sample Includes the Bubble Period (1999-2000)

	Pre-Reform Sample	Post-Reform Sample	Tests of Equality (p-values)
Proceeds	102.2	184.3	0.0001***
Market Capitalization	480.6	601.6	0.0155**
Debt/Equity	0.3058	0.5985	0.0034***
Debt/Assets	0.3207	0.4737	0.0001***
Age	12.6707	21.6053	0.0001***
Initial Return	0.3874	0.1236	0.0001***
Cumulative market return 30 days prior to offering	1.0246	1.0169	0.0009***
Number of Observations	1588	380	

Panel B: Pre-Reform Sample Excludes the Bubble Period (1999-2000)

	Pre-Reform Sample	Post-Reform Sample	Tests of Equality (p-values)
Proceeds	93.5534	184.3	0.0001***
Market Capitalization	307.7	601.6	0.0001***
Debt/Equity	0.3738	0.5985	0.0269**
Debt/Assets	0.3747	0.4737	0.0005***
Age	15.0139	21.6053	0.0001***
Initial Return	0.1667	0.1236	0.0037***
Cumulative market return 30 days prior to offering	1.0302	1.0169	0.0001***
Number of Observations	935	380	

money in the offering.[6] Differences in the means of these characteristics are significant at 5% or 1% level. Results in Panel B confirm that the inclusion of the bubble period in the pre-Reform period does not drive the significant differences in the pre- and post-Reform samples. Additionally, the statistics suggest that there are differences in market sentiment between the two eras. Panels A and B report a significant difference in average initial returns and the cumulative value-weighted market return 30 days prior to the offering. Because valuations are sensitive to investor sentiment, even if firm characteristics were essentially unchanged post-Reform, it would be necessary to control for the effects of sentiment as we are comparing firms over two different sets of market conditions. In unreported results, we expanded our pre-Reform sample to include years 1986-2001 and our post-Reform sample to include years 2003-2011. The results are identical to those reported in Table 1 with the sole exception that leverage ratios do not differ between the pre- and post-Reform samples over this extended data period.

The importance of this evidence to our analysis is that it suggests that some type of selection effect is operative between our pre-Reform and post-Reform samples. The rate at which firms go public (fewer firms do so post-Reform) and the type of firms that do (firms that go public post-Reform are larger, have larger offerings, are more levered, and are older) clearly differ. Thus, to make any valid inferences regarding

[6] Other authors have also noted a size effect related to regulation. Utilizing a Japanese dataset over a period during which several deregulations took place, Takahashi and Yamada (2015) document that prior to deregulation, IPO firms are larger than private firms and smaller following deregulation. Their results are consistent with the notion that regulation prohibits small firms from accessing public capital markets. Dambra, Field and Gustafson (2015) likewise find that the number of small IPOs in the U.S. increased significantly following the passage of the JOBS Act, which substantially reduced regulatory burdens, including those in SOX Section 404, on firms with less than $1 billion in annual revenues.

valuation differences between the samples, and in particular for any effects of board structure compliance on valuations, it is necessary to control for this potential self-selection. The markets in which firms go public also differ with initial returns and market returns prior to offerings lower post-Reform. Thus, it will also be necessary to control for these differences in assessing the effect of Reform on IPO valuations.

Methodology and model

Constructing the matching sample

Determining the impact of Reform on the valuation of IPOs issued post-Reform requires identifying and examining a comparable pre-Reform sample. Previous studies that examine the influence of Reform on various aspects of the IPO market have simply compared the full sample of post-Reform IPOs to all IPOs issued over some time frame prior to the implementation of Reform (e.g. Akhigbe, Martin, and Newman, 2008; Johnston and Madura, 2009; Gao, Ritter, and Zhu, 2013). Unfortunately, this method does not control for the potential self-selection bias that occurred post-Reform. That is, the number of firms and the characteristics of firms that choose to go public is clearly different post-Reform, which suggests some firms, for whatever reason, likely self-selected out of the public markets post-Reform. Thus, in order to compare a characteristic such as valuation pre- and post-Reform, it is necessary to address this potential selection effect.

In addition to selection bias, it is important to control for variations in sentiment across the two markets. Otherwise, it is impossible to determine whether observed changes in IPO valuations post-Reform are the result of the impact of Reform on firms that go public or the result of changes in the pool of IPOs or differences in market sentiment. We follow a widely accepted method of controlling for selection bias due to observable differences between comparison groups by creating a matched sample of pre- and post-Reform firms using propensity score matching. Controlling for relevant differences between the post-Reform ("treated") and pre-Reform ("control") samples, a matched sample can produce an unbiased estimate of the impact of Reform (the "treatment").

Dehejia and Wahba (2002) note that propensity score matching is particularly useful for creating matched samples where the comparison groups differ in a variety of ways as it allows the researcher to match each "treated" firm to a "control" firm on multiple dimensions. When the dimensionality of differences between the treated and control groups is high, other matching methods are problematic because it is difficult to decide upon which dimensions to match and how to weight the relative importance of each matching dimension. Dehejia and Wahba (2002) demonstrate that propensity score matching resolves these problems as it produces a natural weighting scheme that generates unbiased estimates of the treatment effect. In addition, propensity score matching is well-accepted in the corporate finance literature. Several recent studies, including those by Boubaker, Rouatbi and Saffar (2017), Cumming, Grilli and Murtinu (2017), Masulis and Nahata (2011), Lee and Masulis (2011), Kaserer, Mettler and Obernberger (2011), Li and Zhao (2006), Hogan and Lewis (2005), Lee and Wahal (2004), and Villalonga (2004), use it to create matched samples. Please refer to Appendix A.1 for details of the propensity score matching procedure. The final sample resulting from this procedure is 380 post-Reform and 380 pre-Reform IPOs.

Table 2 demonstrates that this selection method produces a pre-Reform control sample with characteristics similar to those of the post-Reform treated sample and virtually identical average propensity scores. Because there are no longer significant differences in average size, proceeds, leverage, age and market sentiment between the two samples, our analysis should be untainted by selection bias driven by

Table 2: Descriptive Statistics: Matched Sample of Pre- and Post-Reform IPOs

This table provides descriptive statistics (i.e. means) for the matched sample of IPOs from 1996-2001 (Pre-Reform) and 2005-2009 (Post-Reform) periods for a number of IPO characteristics. Matching is done by propensity scores. Proceeds and market capitalization are in inflation-adjusted 2009 millions of dollars. It also tests for the equality of the sub-sample means. ***,**, and * indicate significant differences at the 1%, 5%, and 10% levels, respectively.

Differences in Averages

	Pre-Reform Sample	Post-Reform Sample	Tests of Equality	
	Mean	Mean	Difference	p-value
Proceeds	164.8	184.3	19.5	0.537
Market Capitalization	596.2	601.6	5.4	0.9572
Debt/Equity	0.5429	0.5985	0.0556	0.6228
Debt/Assets	0.4484	0.4737	0.0254	0.4183
Age	20.0974	21.6053	1.51	0.4374
Initial Return	0.1104	0.1236	0.0131	0.3609
Cumulative market return 30 days prior to offering	1.0121	1.0169	0.0048	0.1199
Propensity Score	0.2466	0.2485	0.00194	0.8116
Number of Observations	380	380		

Table 3: Yearly Averages of Sample Characteristics

Panel A of this table provides descriptive statistics (i.e. means) on a number of IPO characteristics by year for our propensity score matched sample. Panel B shows the distribution of propensity score pre-Reform matched firms by year compared with the total available pre-Reform population. Proceeds and market capitalization are in inflation-adjusted 2009 millions of dollars.

Panel A: Sample Characteristics by Year

Year	Number of IPOs	Proceeds	Market Capitalization	Debt/ Equity	Age	Initial Return	Cumulative Market Return
1996	89	79.34	269.56	0.59	18.82	0.0798	1.0126
1997	103	75.46	339.91	0.60	22.93	0.0880	1.0178
1998	47	141.81	509.73	0.55	25.60	0.1380	1.0268
1999	68	231.18	1355.64	0.55	18.21	0.1560	1.0123
2000	56	153.82	831.85	0.30	14.09	0.1227	0.9979
2001	17	988.13	2212.86	0.72	21.76	0.1077	0.9795
2005	106	181.33	534.15	0.72	23.78	0.1048	1.0163
2006	112	170.83	599.47	0.43	23.20	0.1229	1.0206
2007	113	150.02	693.04	0.33	15.70	0.1565	1.0127
2008	15	210.64	816.46	2.93	22.40	0.0512	0.9607
2009	34	340.61	1340.95	0.62	28.85	0.1067	1.0449

Panel B: Sample Number of IPOs Relative to Total Number of Issues Eligible for Matching

Year	Sample Number of IPOs	Total Number of Issues	Sample % of Total
1996	89	334	26.6%
1997	103	349	29.5%
1998	47	203	23.2%
1999	68	360	18.9%
2000	56	295	19.0%
2001	17	57	29.8%

observable characteristics and differing market conditions. Thus, our results should be driven by the impact of Reform rather than inherent differences in the characteristics of the pre- and post-Reform samples.

Table 3 details the characteristics of the matched sample by year of issuance. First, the number of pre-Reform sample firms (1996-2001) tends to load slightly more on 1996 and 1997 issues, as shown in Panel A. However, this is not surprising, as 1996 and 1997 have the second and third largest number of issues, and therefore, potential matching firms. Panel B shows that as a percentage of the total number of potential matching issues that occurred in each year, the pre-Reform sample is well distributed. Importantly, the pre-Reform sample is not dominated by bubble period (1999-2000) issuers either in terms of raw numbers or as a percentage of the eligible matches by year. Panel B demonstrates that as a percentage of eligible matching firms by year, fewer sample firms come from the bubble period than any other year. Furthermore, the bubble-period sample firms do not have significantly higher average initial returns or cumulative market returns than issues from other years.

Measuring valuation: The dependent variables

We focus on several accounting ratios as measures of valuation in our analyses. Following other IPO valuation studies (e.g. Kim and Ritter, 1999; Purnanandam and Swaminathan, 2004), we examine both historical (pre-offering) and contemporaneous (post-offering) measures of valuation. Our pre-offering measures include price-to-earnings before interest, taxes, depreciation and amortization (EBITDA), price-to-sales, price-to-earnings, and pre-offering market-to-book ratios. The post-offering measures include price-to-sales, price-to-earnings, post-offering market-to-book, enterprise value-to-EBITDA, and enterprise value-to-sales. Refer to Appendix A.2 for details on the data sources and calculation of each component of the ratios.

One of the weaknesses of prior IPO valuation studies using financial ratios is the need to eliminate firms with zero sales, or negative earnings per share, EBITDA or book values (e.g. a firm with a high price and negative earnings would have a high valuation relative to its earnings, but the price-to-earnings ratio would be highly negative, implying a very low valuation). This is a weakness because it substantially reduces sample size and more importantly may eliminate firms, which could alter the conclusions drawn from an analysis. We do not make such exclusions because they would eliminate young firms and firms in particular industries (e.g. pharmaceutical companies with limited or no historical revenues and high research and development costs). Furthermore, if we make such exclusions, we lose a substantial proportion of our sample. If we eliminate IPOs with zero sales and the firms matched to those IPOs, we lose 8.7% of our sample. Eliminating IPOs with non-positive EBITDA and matched firms removes 54.7% of our sample. If we drop IPOs with non-positive earnings and their matches, we lose 73.7% of our sample. Dropping IPOs with non-positive post-offering book value and the matched firms drops 7.6% of our sample. And if we drop all firms with zero sales, or non-positive EBITDA, earnings or book value, we lose 76.8% of our sample. Hence, we must develop another way of forming the valuation ratios that resolves the negative number problem. Given that we did not match firms on industry when we created our matched sample, we also need to control for industry effects in valuation.

We resolve both problems by creating industry percentile rankings for each of the components of the standard accounting ratios. First, we calculate the percentile rank of each IPO's inflation adjusted market capitalization, enterprise value, EBITDA, sales, earnings, and book value relative to all other firms in the IPO's industry (based on the 2-digit SIC code) in the Compustat universe over all sample years (1996-2001 and 2005-2009). The prices, shares outstanding, and accounting measures of each firm in the IPO's industry are collected from Compustat for each year in the sample, where the prices are those on the date of the firm's fiscal year end. Additionally, since we are comparing prices and accounting variables over several years, all numbers are inflation-adjusted by the Consumer Price Index prior to calculating the percentile rankings. Then we form ratios of the IPO's market capitalization (or enterprise value) percentile ranking to each of its EBITDA, sales, earnings, and book value percentile rankings (both historical and contemporaneous). This method accounts for both variations in valuation by industry and the negative number problem. We use aggregate numbers, rather than per share numbers, to calculate the percentile rankings to ensure that

possible differences in the way that the per share accounting measures and prices are distributed do not taint the results.

In addition to the individual ranking ratios, we create a parsimonious measure of valuation using factor analysis. One benefit of using a common factor is that it is less subject to random measurement errors. Table 4 presents the results of this factor analysis. Panel A gives the correlations of the original valuation ranking variables. Panel B presents the eigenvalues of the correlation matrix. We use the first factor, designated *VALUATION_F1*, which has an eigenvalue of 3.348 and explains 37.2% of the variation in our other valuation ranking measures, as an additional proxy for valuation. Panel C gives the correlations of this factor with the original valuation ranking measures.

Testing the effect of reform compliant board structures

Board structure refers to both the size and the composition of the board and its committees. We collect information on the size of the board and the audit, compensation and nominating committees, as well as the proportion of the board and committees made up of independent, inside, and related directors. Inside directors are those who are employed by the firm at the time of the offering. Related directors are those who have some conflict of interest or relationship with the company that jeopardizes their ability to act independently of management. Post-Reform, each exchange lists such relationships that would prevent a director from qualifying as independent; thus, directors who are neither insiders nor independent are defined as related. Post-Reform, issuers generally name the directors who do and do not qualify as independent under the new listing rules. However, for IPOs issued pre-Reform, we must make determinations of independence by applying the independence requirements of the appropriate listing exchange to the information provided in the prospectus. To the extent that issuers fail to report relationships relevant to the determination of independence, we overestimate the extent of board and committee independence pre-Reform. In addition to the previous board characteristics, we create two dummy variables, the first of which equals one if a venture capitalist holds a board seat and zero otherwise (VCBoardSeat), and the second of which equals one if a non-VC institutional investor holds a board seat and zero otherwise (InstBoardSeat).

Using the information we collect on board structure, we estimate the extent to which issuers, both pre- and post-Reform, are compliant with the independence rules of the exchanges at the time of the offering with a Compliance Index that is formed following a method like that implemented by Chhaochharia and Grinstein (2007). This index ranges in value from zero to four, where firms complying with none of the board independence requirements have Compliance Index values of zero and those in complete compliance have index values of four. Refer to Appendix A.3 for details on the calculation of the Compliance Index.

Additionally, we code a ControlledCo dummy variable equal to one if more than 50% of the voting power for the election of directors is held by an individual, group or another company, and equal to zero otherwise. The controlled company status is important because controlled companies are exempt from all the board independence requirements except the requirement that the board have a completely independent audit committee made up of at least three members.[7]

Control variables

The control variables included in the models for firm valuation can be grouped into ownership structure variables and other general control variables found by previous studies to impact firm valuations. We control for ownership structure because, in addition to the changes required by Reform, ownership structure is a method of monitoring firm behavior. The additional general control variables include offering proceeds, the ratio of secondary share proceeds to total offering proceeds, firm age, research and development and advertising expenditures for the last full fiscal year prior to the offering, total assets before the offering, underwriter rank, and a dummy variable equal to one if the firm lists on the Nasdaq and zero otherwise. Finally, we include industry control variables to capture any variations in firm values across

[7] We explore the possibility that status as a controlled firm affects the relation between the *Compliance Index* and valuation by including an interaction between *ControlledCo* and the *Compliance Index*. The interaction effect is not significant and has no effect on the results, so it is dropped from the reported analysis.

Table 4: Factor analysis

This table provides information related to the extraction of the common valuation factor using factor analysis. Panel A presents the correlation of the original valuation ranking variables. Panel B presents the eigenvalues of the correlation matrix and proportion of variation explained. Panel C provides the table of correlations between the extracted valuation factor and the original valuation ranking ratios.

Panel A: Correlation Matrix for Valuation Measures

Pearson Correlation Coefficients, N = 760, Prob ? |r| under HO: Rho = 0

LN Valuation Rank	Price/EBITDA$_{pre}$	Price/Sales$_{pre}$	Price/Earn$_{pre}$	Market/Book$_{pre}$	Market/Book$_{post}$	Pr/Earn$_{post}$	Price/Sales$_{post}$	EntVal/EBITDA	EntVal/Sales
Price/EBITDA$_{pre}$	1								
Price/Sales$_{pre}$	0.50442	1							
	<.0001								
Price/Earnings$_{pre}$	0.63244	0.25491	1						
	<.0001	<.0001							
Marker/Book$_{pre}$	0.00463	-0.05687	0.09807	1					
	0.8986	0.1172	.0068						
Market/Book$_{post}$	-0.06415	-0.06291	0.00517	0.6385	1				
	0.0772	0.0831	0.8868	<.0001					
Price/Earnings$_{post}$	0.57181	0.36461	0.5837	-0.02299	-0.0852	1			
	<.0001	<.0001	<.0001	0.5268	0.0188				
Price/Sales$_{post}$	0.38687	0.72254	0.21135	-0.05395	-0.0637	0.4784	1		
	<.0001	<.0001	<.0001	0.1373	0.0793	<.0001			
EntVal/EBITDA	0.65988	0.31607	0.41811	0.02898	-0.01445	0.15566	-0.18052	1	
	<.0001	<.0001	<.0001	0.425	0.6909	<.0001	<.0001		
EntVal/Sales	0.29931	0.64142	0.14644	-0.01246	-0.01154	-0.00325	0.03	0.76233	1
	<.0001	<.0001	<.0001	0.7316	0.7508	0.9287	0.4088	<.0001	

Panel B: Eigenvalues of the Correlation Matrix

Factor Number	Eigenvalue	Proportion	Cummulative
1	3.34800937	0.3720	0.3720
2	1.72663805	0.1918	0.5638
3	1.58999595	0.1767	0.7405
4	1.21161508	0.1346	0.8751
5	0.36738835	0.0408	0.9160
6	0.35755782	0.0397	0.9557
7	0.33004591	0.0367	0.9924
8	0.06874947	0.0076	1.0000
9	0.00000000	0.0000	1.0000

Panel C: Correlation of Factor 1 with the Natural Log of the Valuation Ratios

	Factor 1
Price/EBITDA$_{pre}$	0.86533
Price/Sales$_{pre}$	0.78154
Price/Earnings$_{pre}$	0.68365
Market/Book$_{pre}$	-0.02863
Market/Book$_{post}$	-0.09635
Price/Earnings$_{post}$	0.65875
Price/Sales$_{post}$	0.53859
EntVal/EBITDA	0.66372
EntVal/Sales	0.58853

industries that may not be captured in the dependent variables.[8] Refer to Appendix A.4 for a more thorough discussion of the control variables utilized in the model presented in Equation 1 below.

Methodology

In order to test the impact of the board structure provisions of Reform and the resulting changes to corporate governance on IPO valuations (Hypothesis 1), we regress the industry ranking ratio of each accounting multiple on the Compliance Index and each of the control variables previously discussed. The right-hand-side of the regression for each accounting multiple is identical to that of the price-to-EBITDA ranking ratio below:

$$\frac{Industry\ percentile\ ranking\ of\ price_i}{Industry\ percentile\ ranking\ of\ EBITDA_i} = \alpha_0 + \beta_1 Compliance\ Index_i + \beta_2 CEOHold_i + \beta_3 OtherMgrHold_i$$
$$+\beta_4 VCHold_i + \beta_5 InstHold_i + \beta_6 OtherOutsideHold_i + \beta_7 VCBoardSeat_i + \beta_8 InstBoardSeat_i$$
$$+ \beta_9 ControlledCo_i + \beta_{10} CEOFounder_i + \beta_{11} ExecFounder_i + \beta_{12} ChairFounder_i + \beta_{13} BlockFounder_i$$
$$+\beta_{14} DirFounder_i + \beta_{15} Ln(Proceeds)_i + \beta_{16} SecRatio_i + \beta_{17} Nasdaq_i + \beta_{18} Age_i + \beta_{19} DualClass_i$$
$$+\beta_{20} UseProcOpEx_i + \beta_{21} RDExp_i + \beta_{22} AdvExp_i + \beta_{23} Assets_i + \beta_{24} UWRank_i + \sum_{j=25}^{n} \beta_j IndDummy + \varepsilon \quad (1)$$

To test Hypothesis 2, the general effect of Reform on IPO valuations, we simply replace the *Compliance Index* in Equation 1 with a *Post-Reform* dummy variable (taking on the value of one if the IPO was issued post-Reform and zero otherwise) and this produces Equation 2.

We do not include the *Compliance Index* and *Post-Reform* dummy variables in the same equation because they are highly correlated (0.49 Pearson correlation coefficient, significant at the 1% level) and preliminary tests suggest that this collinearity affects the results. We run the regressions for Equations 1 and 2 for each ratio and the extracted valuation factor and for the natural log of each ratio. All regression models report White's (1980) heteroskedasticity-consistent standard errors.

Findings

Univariate analysis of board structure and ownership

We examine average differences in board and ownership structure pre- and post-Reform for our matched sample. Since Reform mandated changes in board composition, we expect there to be significant differences in board and committee independence across the samples. Table 5 confirms this expectation. The *Compliance Index* is 1.22 for the average pre-Reform issuer and 2.73 for the average post-Reform IPO. The difference in the means is significant at the 1% level. As would be expected, the individual components of the Compliance Index also vary across the samples.

The percentage of the board composed of independent directors increased from 48.73% for the average pre-Reform issuer to 61.67% for the average post-Reform issuer. The percentage of firms with a majority of independent directors also increased from 45.8% in the pre-Reform sample to 71.3% in the post-Reform sample. The percentage of independent committee members likewise increased significantly. The largest change occurred in the percentage of independent nominating committee members, from 4.54% pre-Reform to 77.68% post-Reform. This is primarily because most pre-Reform issuers had no nominating committee and what few that did gave such responsibilities largely to insiders and otherwise related directors. The percentage of firms with completely independent committees also increased significantly post-Reform; the percentage of firms with a completely independent audit committee, compensation committee, and nominating committee increased 35.3%, 25%, and 65.3%, respectively. In short, for the overall board and

[8] We also attempt to include dummy variables to capture yearly effects. However, these variables are correlated with our variables of interest, the *Post-Reform* dummy variable and the *Compliance Index*. In addition, some of the sample partitions include as few as 40 observations and including additional control variables becomes problematic. Thus, they have been omitted because of multicollinearity and sample size concerns.

for each committee, independent directors represented a statistically significantly higher proportion post-Reform. Since we are comparing a matched sample, the evidence suggests that Reform did significantly change board structure and forces firms away from their preferred unconstrained board structures. While Reform gave no specific directives regarding board seats held by venture capitalists and other institutional investors, we find that the proportion of board seats held by venture capitalists increases while the proportion of board seats held by other institutional investors decreases post-Reform.

Panel B presents information on founder involvement and ownership structure. The picture it paints is that firms that go public post-Reform have much lower private benefits of control than their pre-Reform matched counterparts. Founders are less likely to be the CEO and more likely to be a non-executive, non-chair, non-director blockholder post-Reform. Thus, post-Reform the founder is less likely to be directly involved in the day-to-day operations of the firm. Also, CEOs and non-CEO managers have significantly lower stockholdings post-Reform. The holdings of all managers fall from 19.75% to 9.34%, those of CEOs fall from 12.2% to 5.8% and the holdings of non-CEO managers fall from 7.79% to 3.61%. On the other hand, the holdings of venture capitalists rise from 17.09% to 32.69%. A likely explanation for the increase in venture capitalist (VC) holdings and board representation post-Reform is that those firms that choose to go public following Reform are only doing so because of the presence of the VCs, who push the firms to go public so they can exit from their investments. That is, in the absence of significant VC control, these firms might not otherwise choose to go public because the costs outweigh the benefits. For the firms with significant VC holdings that go public, the private benefits of control are likely already relatively low because of the presence of the VCs in the first place. Thus, post-Reform insiders have a significantly smaller interest in and venture capitalists, presumably interested in cashing out, have a much larger interest in firms that choose to go public. Or, the private control benefits of firms that choose to go public post-Reform are significantly smaller than their pre-Reform matched-sample counterparts. This clearly suggests that private benefits of control, which have been diminished by the regulatory corporate governance imposed by Reform, do affect the type of firm choosing to go public post-Reform.

Predicted Compliance Index

The fundamental objective of this study is to evaluate the impact on the valuation of firms that were forced to alter their board structures as a result of Reform. Undoubtedly, our sample mixes firms that would have chosen to construct their boards in the same manner that Reform requires them to (call this Group 1) with firms that have had to change their board structure from what it would have been if the firm were not constrained. For firms that have had to change their board structure, some may have been required to do so in a value increasing way (Group 2) and some have been required to do so in a value decreasing way (Group 3). Again, the full sample may be a mixture of all three groups and the board structure consequences and other effects of Reform would be expected to differ across the three groups. As a result, analysis on the full sample may produce results that are largely ambiguous because of these differing effects.[9] Thus, we attempt to separate firms based on how, or if, Reform changed the firms' unconstrained board structure.[10]

To accomplish this, we create subsamples using a linear factor approach to predict the post-Reform firms' compliance index. Specifically, we build a predictive model of our pre-Reform firms' compliance indexes from a set of variables that would be expected to be related to board structure and then take the coefficients obtained to build a predictive model for the board structure of post-Reform firms.

Our predictive variables are drawn from the corporate finance literature that has identified several factors affecting optimal board structure. Insiders on a board are valuable where firm-specific information is important for monitoring and strategic advising. Proxies for these types of firms include measures of growth opportunities available to the firm, such as R&D expenditures, measures of firm complexity, such as firm size and age (Fama and Jensen, 1983; Lehn, Patro and Zhao, 2009; Boone, Field, Karpoff, and Raheja,

[9] Unreported analysis on the full sample confirms this expectation. The *Compliance Index* is largely insignificant across all valuation measures. The *Post-Reform* variable is negative and significant in three instances, but only at the 10% level in two of those regressions.

[10] We thank an anonymous referee for suggesting this line of analysis.

Table 5: Descriptive Statistics: Matched Sample of Pre- and Post-Reform IPOs
Differences in Averages: Board and Ownership Structure

This table provides descriptive statistics (i.e. means) for the matched sample of IPOs from 1996-2001 (Pre-Reform) and 2005-2009 (Post-Reform) periods for a number of IPO characteristics related to board structure (Panel A), founder involvement (Panel B), and ownership structure (Panel C). Matching is done by propensity scores. It also tests for the equality of the sub-sample means. ***,**, and * indicate significant differences at the 1%, 5%, and 10% levels, respectively.

	Pre-Reform Sample	Post-Reform Sample	Tests of Equality	
	Mean	Mean	Difference	p-value
Panel A: Board Structure				
Compliance Index	1.2158	2.7263	1.5105	0.0001***
% of Board Comprised of Independent Directors	0.4873	0.6167	0.1294	0.0001***
% of Independent Audit Committee Members	0.6648	0.8485	0.1837	0.0001***
% of Independent Compensation Committee Members	0.6463	0.8339	0.1876	0.0001***
% of Independent Nominating Committee Members	0.0454	0.7768	0.7314	0.0001***
% of Firms with a Majority Independent Board	0.4579	0.7132	0.2553	0.0001***
% of Firms with Independent Audit Committee	0.2526	0.6053	0.3526	0.0001***
% of Firms with Independent Compensation Committee	0.4868	0.7368	0.2500	0.0001***
% of Firms with Independent Nominating Committee	0.0184	0.6711	0.6526	0.0001***
VCBoardSeat	0.4789	0.7368	0.2579	0.0001***
InstBoardSeat	0.3632	0.2500	-0.1132	0.0007***
Panel B: Founder Involvement				
CEO Founder	0.3763	0.3105	-0.0658	0.0563*
Executive Founder	0.0711	0.0921	0.0211	0.2897
Chairman Founder	0.0816	0.0579	-0.0237	0.2004
Director Founder	0.0263	0.0447	0.0184	0.1706
Other Blockholder Founder	0.0368	0.0947	0.0579	0.0013***
Panel C: Ownership Structure				

MgrHold	0.1975	0.0934	-0.1040	0.0001***
CEOHold	0.1220	0.0580	-0.0640	0.0001***
OtherMgrHold	0.0779	0.0361	-0.0418	0.0001***
VCHold	0.1709	0.3269	0.1560	0.0001***
InstHold	0.1638	0.1086	-0.0552	0.0008***
OtherOutsideHold	0.0653	0.0498	-0.0155	0.0637*
% of Firms with VC Backing	0.5184	0.7974	0.2789	0.0001***
% of "Controlled" Firms	0.2579	0.2184	-0.0395	0.202
Number of Observations	380	380		

2007; Coles, Daniel and Naveen, 2008) and measures associated with less influential CEOs, such as non-founder CEOs. Additionally, several theoretical and empirical studies in the governance literature suggest other firm-specific factors that lead to a higher or lower proportion of independent directors. Studies by Hermalin and Weisbach (1998), Baker and Gompers (2003) and Boone, Field, Karpoff, and Raheja (2007) suggest that board structure results from negotiations between a firm's outside directors and its CEO. Thus, the more powerful the CEO, as proxied by CEO ownership in the firm, the lower the proportion of independent directors. On the other hand, measures of constraints on insiders' influence, such as venture capital presence and underwriter rank, lead to a higher proportion of independent directors. In addition to many of the above referenced factors, Linck, Netter, and Yang (2008), find that leverage also significantly positively influences board independence.

The model for the Predicted Compliance Index, with the coefficient values obtained, is thus:

$$Predicted\ Compliance\ Index = 1.19 + 0.002 * R\&D\ Expenditures - 0.00004 * MarketCap(Offer\ Price) \\ -0.004 * Age + 0.04 * CEO\ Founder\ Dummy + 0.15 * CEO\ Ownership + 0.38 * VC\ Backed \\ +0.02 * Underwriter\ Rank - 0.56 * Debt\ to\ Assets \quad (3)$$

We then calculate the difference between the *Actual Compliance Index* and the *Predicted Compliance Index* for our post-Reform sub-sample and sort the post-Reform firms on this difference. We then examine subsamples of the lowest tercile of the differences (*Actual – Predicted* of 1.10 or less) and their pre-Reform matched counterparts and the highest tercile of differences (*Actual – Predicted* of 2.45 or more) and their pre-Reform matches. The lowest tercile subsample effectively constitutes Group 1 type firms (i.e. their board structure was not radically altered by Reform). We would expect the compliance index to have little effect in this sub-sample. The highest tercile subsample are firms whose board structure was changed by Reform and, presumably, constitutes some mixture of Group 2 (value increasing changes) and Group 3 (value decreasing changes) firms. However, by separating out Group 1 firms, we may be able to measure whether Group 2 or Group 3 firms are more predominant in this subsample and shed light on the effects of Reform. If the *Compliance Index* coefficient is significantly positive, Group 2 firms predominate, if the *Compliance Index* coefficient is significantly negative, Group 3 firms predominate. In the former case, regulatory corporate governance adds value; in the latter case, it destroys value, and, hence, provides some evidence that Reform may be deterring some IPOs.

The effect of the *Post-Reform* dummy variable across the three groups is more complex to predict as it is measuring numerous benefits and costs of Reform, although one possibility is that the general effects of Reform are correlated with the effects of compliance. On the cost side there are the costs of compliance, of which Section 404 is one of the most monetarily costly. On the benefit side are informational benefits resulting from the rules applying to auditors, additional reporting, the internal control audit of Section 404 and the benefits of extending liability to executives by way of forcing them to certify the accuracy of financial information reported to shareholders.

In addition to examining Group 1 separately from Groups 2 and 3, we also consider the differential impact of Reform on IPOs of different sizes. As previously noted, firms with less than $75 million in public float were exempted by the SEC from complying with Section 404 of SOX, which requires a management report on the internal control systems of the firm, management's attestation of the report, and an auditor's attestation. Section 404 is by far the most monetarily costly aspect of SOX compliance. As the costs associated with compliance are largely fixed while the marginal benefit is likely a function of firm size and since IPO firms are small relative to the universe of firms subject to Section 404, their costs are likely higher relative to the benefits derived. One reason to believe this to be true is that the U.S. government exempted small firms (those with less than $75 million in public float) from Section 404. The SEC extended this exemption from Section 404 several times, the last through June 30, 2010. The Dodd-Frank Wall Street Reform Act and Consumer Protection Act made the exemption permanent from Section 404(b), which requires the auditor attestation of the management report on internal control, by adding Section 404(c) to SOX.

To test the impact of this on our results, we create separate samples of firms. Small firms have less than $75 million in public float and hence the post-Reform firms in this sub-sample are not required to comply with SOX Section 404 during our data period. Large firms have more than $75 million and hence the post-Reform

firms in this subsample are required to comply with SOX Section 404 during our data period. The implication is that SOX compliance costs will be higher for the post-Reform firms in the large firm sample, which might increase the negative effect of the post-Reform dummy on these firms. We then repeat our analyses on these separate small firm and large firm subsamples.

Results

The analysis involving the Group 1 subsample (i.e. low *Actual – Predicted* difference, Reform did not move board structure) appears in Table 6 for the full Group 1 subsample and the large and small firm partitions of this subsample. The analysis involving the Group 2/3 subsample (i.e. high *Actual – Predicted* difference, Reform changed board structure) appears in Table 7 for the full Group 2/3 subsample and the large and small firm partitions of this subsample.[11]

The results of Table 6 indicate that the *Compliance Index* is generally insignificant; there are no significant coefficients for the full sample and only the coefficient in the market-to-book value regression is significant (negatively, at the 10% level) in the large firm subsample. This is as expected since board structure is little changed among Group 1 IPOs. Things are a little different for the small firm subsample. The coefficients on the *Compliance Index* in the price-to-sales and extracted valuation factor regressions are negative and significant at the 1% level, although the coefficient is positive and significant at the 1% level in the market-to-book value regression.

The entirety of the Group 1 results suggests that firms receive little to no benefit in terms of investor valuation from being more compliant with the listing requirements resulting from Reform, even if Reform did not significantly alter their unconstrained board structures. If the mandated board structure following Reform was optimal for all publicly listed firms, we would expect a consistently and significantly positive coefficient on the *Compliance Index* across all valuation measures, particularly for the Group 1 firms. The lack of a positive relationship is consistent with the "one size does not fit all" board structure literature. While there is one instance in which the *Compliance Index* is positive and significant in the small firm subsample, there are two other instances in which it is negative and significant. Thus, there is some mixed evidence that compliance with the board structure provisions of Reform may have reduced valuations for the smallest IPO firms, even among those firms expected to voluntarily structure their boards in a compliant way.

The impact of the general consequences of Reform (e.g., costs of compliance, change in the private benefits of control and changes in the cost of capital) on IPO valuations should be captured by the *Post-Reform* dummy variable. The coefficient is negative and significant at the 10% level in only the price-to-earnings regression for the full Group 1 sample. It is never significant in the small firm subsample regressions and is negative and significant at the 5% level in three regressions for the large firm subsample (price-to-sales, enterprise-value-to-sales and the extracted valuation factor). This result provides evidence that firms not subject to Section 404 of SOX fare better than those that are, even if board structure is not significantly altered by Reform, and is consistent with claims that the compliance costs of Section 404 are a significant issue for many relatively small firms that are forced to comply with them. Taking the results on the *Compliance Index* and *Post-Reform* dummy together, we find no clear benefit from the regulatory corporate governance provisions of Reform and at least some evidence that valuations fall post-Reform among Group 1 firms, particularly for those subject to the costly Section 404 provision of SOX.

The results for firms where Reform moved board structure significantly in Table 7 indicate that this subsample is dominated by Group 3 firms (i.e. compliance reduces value). The negative results are stronger than for the full sample in Table 6 (Group 1). The *Compliance Index* is negatively significant for the full Group 2/3 sample for the price-to-sales ratio, market-to-book ratio and the enterprise value-to-sales ratio (all at the 5% level) and for the small-firm subsample for the price-to-earnings ratio (5% level), market-

[11] The results including the coefficients and t-statistics on the control variables are omitted for the sake of space. Results including the control variables and detailed discussions are available upon request.

Table 6: Difference between Actual and Predicted Compliance Index is in the Bottom One-Third in Sample (Group 1)
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is from the lowest tercile of the full post-Reform sample (i.e. the difference is 1.10 or less). Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 252 for the full sample, 40 for the small firm sub-sample, and 212 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	0.03	(0.48)	-0.06	(-1.04)	0.08	(1.33)	-0.07	(-1.48)	0.02	(0.41)	-0.07	(-1.16)	-0.01	(-0.19)
Small Firm	0.06	(0.49)	-0.53	(-3.14)***	0.15	(0.96)	0.27	(2.81)***	0.33	(0.76)	-0.27	(-0.58)	-0.48	(-5.75)***
Large Firm	0.00	(0.06)	-0.03	(-0.55)	0.04	(0.56)	-0.10	(-1.77)*	0.01	(0.15)	-0.03	(-0.50)	-0.02	(-0.35)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.18	(-1.40)	0.05	(0.43)	-0.24	(-1.84)*	0.03	(0.26)	-0.19	(-1.48)	0.03	(0.30)	-0.09	(-0.88)
Small Firm	-0.03	(-0.29)	0.29	(1.03)	-0.20	(-1.11)	0.03	(0.20)	0.31	(0.75)	0.63	(1.45)	0.06	(0.33)
Large Firm	-0.17	(-1.15)	-0.20	(-2.18)**	-0.23	(-1.44)	0.12	(0.87)	-0.16	(-1.05)	-0.18	(-2.03)**	-0.24	(-2.11)**

Table 7: Difference between Actual and Predicted Compliance Index is in the Top One-Third in Sample (Group 2/3)
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is in the highest tercile of the sample (i.e. the difference is 2.45 or more). Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 252 for the full sample, 70 for the small firm sub-sample, and 182 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.03	(-0.81)	-0.07	(-2.24)**	0.01	(0.16)	-0.06	(-2.04)**	-0.07	(-1.13)	-0.11	(-2.05)**	-0.04	(-0.98)
Small Firm	-0.08	(-0.97)	-0.14	(-0.89)	-0.16	(-2.06)**	-0.11	(-1.83)*	0.34	(1.45)	0.27	(1.03)	-0.23	(-2.15)**
Large Firm	-0.03	(-0.72)	-0.04	(-0.81)	0.05	(0.90)	-0.07	(-1.41)	-0.10	(-1.49)	-0.10	(-1.60)	0.02	(0.33)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.12	(-1.03)	-0.32	(-2.74)***	-0.21	(-1.51)	-0.17	(-2.05)**	-0.10	(-0.51)	-0.30	(-1.62)	-0.26	(-2.13)**
Small Firm	-0.33	(-1.18)	-0.70	(-2.09)**	-0.63	(-2.48)**	-0.55	(-2.28)**	2.19	(3.14)***	1.82	(2.34)**	-1.49	(-4.84)***
Large Firm	-0.15	(-1.08)	-0.17	(-1.19)	-0.14	(-0.81)	-0.22	(-1.76)*	-0.32	(-1.50)	-0.34	(-1.66)*	-0.12	(-0.81)

to-book ratio (10% level) and extracted valuation factor (5% level). It is insignificant in all the large-firm subsample regressions.

The *Post-Reform* dummy displays a similar pattern with negative significance for the full subsample for the price/sales ratio (5% level), the market/book ratio (5% level) and the extracted valuation factor (5% level) and for the large firm sub-sample for the market/book ratio (10% level) and the enterprise value/sales ratio (10% level). However, the negative impact of the *Post-Reform* dummy is more prevalent within the small-firm subsample; the coefficient on this variable is negative and significant at the 5% level in the price/sales, price/earnings, and market/book value regressions and negative and significant at the 1% level for the extracted valuation factor. On the other hand, it is positive and significant for the enterprise value/EBITDA (1% level) and enterprise value/sales (5% level) ratios in the small-firm subsample. Another point of interest is that the *Post-Reform* dummy variable was insignificant across all valuation measures for the Group 1 small-firm subsample. Despite being exempt from Section 404, small firms whose boards are most significantly altered by the governance provisions of Reform (Group 2/3) are also more negatively impacted by the general provisions of Reform. We regard this as an important finding, particularly because it likely serves as another explanation for the dramatic drop in the number of small-firm IPOs post-Reform.

For the Group 2/3 sample, particularly the full sample and small-firm subsample, the evidence strongly supports a negative relationship between IPO valuations and the *Post-Reform* variable, suggesting that the effects of Reform outside of the explicit governance requirements have reduced valuations. This result is consistent with hypothesis H_{2B}; that is, the costs of compliance and/or the impact on the private benefits of control outweigh any positive effects Reform may have on firms' costs of capital. Likewise, when Reform alters board structure, there is evidence that the *Compliance Index* reduces valuations within the full sample and small firms are most negatively impacted. Together these effects may deter IPO issuance at the margin.

The implication of these findings is that Reform poses binding constraints on IPO firms, particularly small equity-issuing firms. Lawmakers appear to recognize these issues; the Jumpstart Our Business Startups (JOBS) Act of 2012 sought to reduce the registration, reporting and accounting burdens on small issuers (known as emerging growth companies or EMGs). Dambra, Field and Gustafson (2015) provide evidence that the JOBS Act has been beneficial to EMGs, leading to a 25% increase in yearly IPO activity. However, more work needs to be done; the results of this study combined with those of Rhodes (2018) and Wintoki (2007) suggest that the board structure constraints of Reform, which the JOBS Act did nothing to modify, are detrimental to some IPO firms. Accordingly, it makes sense to either modify the board structure constraints for small issuers or to repeal them altogether. If lawmakers ultimately relax these requirements, future studies may focus on how board structures are altered and the resulting valuation effects.

Robustness checks

We perform two robustness tests to address the potential impact of the financial crisis on the valuations of our post-Reform sample. First, we incorporate a financial crisis dummy, *Fin Crisis Dummy*, which is equal to one if the IPO took place between August 2007 and December 2008 and zero otherwise, into the analyses reported in Tables 6 and 7. Second, we exclude the IPOs that occurred during the financial crisis (between August 2007 and December 2008) as well as their pre-Reform matched counterparts and re-conduct the analysis. Detailed discussion and the tabulated results of the robustness tests appear in Appendix B.

The results on the Group 1 subsample are not dramatically altered as a result of these exclusions (see Tables B.1 and B.3 of Appendix B). The Group 2/3 results are more substantially affected (see Tables B.2 and B.4 of Appendix B). Specifically, the negative impact of the *Compliance Index* on the small-firm subsample is tempered, although the negative effect is more pronounced in the large-firm subsample with the elimination of the financial crisis IPOs. The results on the *Post-Reform* variable still demonstrate a negative effect on the full Group 2/3 sample and the small-firm subsample. Overall, the results of the robustness tests are consistent with the results presented in the main analysis: where board structures are more significantly altered by Reform, there is more evidence of a negative effect on IPO valuations and these firms are more negatively impacted by the general effects of Reform outside of the board structure provisions.

Conclusion

Utilizing a new methodology for measuring valuations, we find significant differences in board structure pre- and post-Reform in our matched sample of firms, suggesting that the board structure provisions of Reform impose binding constraints on issuers, but find virtually no evidence that compliance with these costly listing requirements increases firm valuations. We find some limited evidence of a negative effect of compliance with the board structure provisions of Reform. This negative effect is more pronounced among those firms whose board structures were most significantly altered by Reform and among small firms in our sample. Since the regulatory corporate governance structures imposed by Reform place significant constraints on private benefits of control, they may prove a significant inhibition to going public in firms where those rights are significant to insiders. We find that in our matched sample post-Reform IPO firms have much lower insider ownership and greater venture capital ownership than pre-Reform matched firms, suggesting that post-Reform firms that go public do exhibit lower control benefits. The corresponding absence of higher valuations post-Reform suggests that private benefits of control do not necessarily represent priced agency costs.

With respect to the general effects of Reform, we find some evidence of a negative effect. When board structures are least altered by Reform (Group 1 firms), the negative effect is concentrated among those firms subject to SOX Section 404. Alternatively, when board structures are more significantly altered by Reform (Group 2/3 firms), the negative effect is concentrated among small firms exempt from Section 404. While there is some evidence that enterprise value-based ratios are higher post-Reform among firms not subject to Section 404, the weight of the evidence even among these exempt firms suggests that the mandates of Reform outside of the board structure provisions have negatively impacted IPO valuations.

We provide new evidence that while the regulatory corporate governance provisions of Reform constitute a significant interference with private benefits of control for firms that go public, they provide limited if any valuation benefit to such firms. In addition, we reveal that Reform, in general, appears to have a negative effect on IPO valuations, particularly among firms whose board structures are most significantly altered, but may have some positive effect on IPO valuations for firms that are exempt from SOX Section 404. Our results suggest that the firms that have opted out of the IPO market are those where the private benefits of control are likely the highest, who would have to alter their board structure in a value-decreasing manner, and for whom the monetary costs of compliance are likewise most significant.

The policy implications of these findings are that the board structure requirements of Reform should either be modified for small issuers or repealed altogether. While lawmakers have made steps in the right direction with passage of the 2012 JOBS Act to relax some of the more costly provisions of Reform, nothing has been done to address the constraints of the board structure mandates. If policymakers eventually relax these requirements, future studies may evaluate the resulting changes in board structures, private benefits of control, and the rates at which firms access public equity markets.

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Appendix

Appendix A: Methodology and Model

A.1 Propensity Score Matching Procedure

We use a propensity score matching procedure to create a matched sample of pre-Reform firms for each post-Reform firm in our sample. This begins with estimating the propensity score of each of the 380 post-Reform and 1588 pre-Reform issues from a logistic regression predicting the probability that the IPO would have been issued post-Reform. In this regression, the dependent variable equals one if the IPO was issued following the implementation of Reform and zero if it was issued before Reform. Since we need to control for size, offering proceeds, leverage, age and market sentiment, the independent variables in the regression are inflation-adjusted market capitalization at the time of the offering, inflation-adjusted proceeds, the debt-to-equity and debt-to-assets ratios at the first reporting date following the offering, firm age at the time of the offering, the issue's initial return, and the cumulative value-weighted market return for the 30 trading days prior to the offering. We deliberately do not control for pre-offering ownership structure in our propensity score matching regression because we wish to determine whether the private benefits of control, as proxied by ownership structure, differ pre- and post- Reform. Then, because we sample from the pre-Reform control sample without replacement and since the resulting sample could be sensitive to the order in which sampling is conducted, we randomize both the pre-Reform and post-Reform samples (as suggested by Dehejia and Wahba, 2002). Finally, using the propensity scores from the first step, we match each post-Reform IPO to its nearest neighbor from the pre-Reform sample. We then hand collect the data necessary to complete this final sample, which is comprised of 380 post-Reform and 380 pre-Reform IPOs.[12]

A.2 Measuring valuation: The dependent variables

We utilize several accounting ratios as measures of valuation in our analyses. Following other IPO valuation studies (e.g. Kim and Ritter, 1999; Purnanandam and Swaminathan, 2004), we examine both historical (pre-offering) and contemporaneous (post-offering) measures of valuation. Our pre-offering measures include price-to-earnings before interest, taxes, depreciation and amortization (EBITDA), price-to-sales, price-to-earnings, and pre-offering market-to-book ratios. The post-offering measures include price-to-sales, price-to-earnings, post-offering market-to-book, enterprise value-to-EBITDA, and enterprise value-to-sales.

The price in each of the ratios is the firm's market capitalization and is calculated by multiplying the firm's offering price by shares outstanding following the offering.[13] Historical EBITDA, sales and earnings per share are collected from income statements in the prospectus and are measured over the most recent full fiscal year prior to the offering. Earnings per share are converted into aggregate earnings by multiplying the per share values by shares outstanding prior to the offering. Book value pre-offering is the book value of shareholder equity taken from the most recent balance sheet prior to the offering. Book value post-offering is calculated as the sum of pre-offering book value of equity and the primary offering proceeds. Contemporaneous sales, earnings per share and EBITDA are taken from Compustat for the first reporting period following the offering. Earnings per share are converted into aggregate earnings by multiplying the per share values by shares outstanding after the offering. Enterprise value is calculated as (market value of equity + book value of debt – cash); it is calculated after the offering where the market value of equity is the product of the offering price and shares outstanding following the offering, and the book value of debt and cash are taken from Compustat for the first reporting period following the offering. Kim and Ritter (1999) suggest examining the enterprise value-to-EBITDA variable because it allows comparisons between firms

[12] Note, our approach is not perfectly consistent with the theoretical foundations of propensity score matching because the independent variables are not truly "predictive" of whether an IPO is issued post-Reform or not (i.e. they are correlations, not causations). Nonetheless, we argue that this approach is operationally valid because it accomplishes the selection of a matching control firm in an arguably less arbitrary way than simply choosing one or two characteristics upon which to match.

[13] For robustness, we also use the first trading day's closing price in place of the offering price in calculating the percentile ranking ratios. The results reported in the paper are based on the offering price, but the results based on the first trading day's closing price are basically identical to those reported.

with varying degrees of leverage, as enterprise value is invariant to the cash raised by the offering and EBITDA is unaffected by leverage.

In order to deal with the negative number problem that plagues the standard accounting-based financial ratios, we create valuation measures based on the industry percentile rankings of each component of these financial ratios. As discussed in the paper, we first calculate the percentile rank of each IPO's inflation adjusted market capitalization, enterprise value, EBITDA, sales, earnings, and book value relative to all other firms in the IPO's industry (based on the 2-digit SIC code) in the Compustat universe over all sample years (1996-2001 and 2005-2009). Then we form ratios of the IPO's market capitalization (or enterprise value) percentile ranking to each of its EBITDA, sales, earnings, and book value percentile rankings (both historical and contemporaneous). Forming the valuation measures in this way controls for variations in valuation by industry and the negative number problem. We use aggregate numbers, rather than per share numbers, to calculate the percentile rankings to ensure that possible differences in the way that the per share accounting measures and prices are distributed do not taint the results.

If a firm has a relatively high price and negative earnings, then its industry price percentile ranking will be relatively high while its industry earnings percentile ranking will be relatively low and the ratio of the two rankings will be relatively high. Thus, the high ratio accurately reflects the firm's high valuation. To see this more clearly, consider a numerical example. We are interested in calculating the price-to-earnings ratio of IPO A using the method just described. IPO A's inflation-adjusted market capitalization is $80 million and there are three other firms in IPO A's industry over the sample period: Firm B with an inflation-adjusted market cap of $75 million, Firm C with an inflation-adjusted market cap of $70 million, and Firm D with an inflation-adjusted market cap of $50 million. IPO A's aggregate inflation-adjusted earnings over the year prior to the offering were -$14 million. Firms B, C, and D have inflation-adjusted earnings of $10 million, $12 million, and $9 million, respectively. Intuitively, we know that IPO A has the highest valuation relative to its earnings, followed by Firm B, Firm C, and then Firm D. However, if we simply take IPO A's price-to-earnings ratio, we will get -5.71, suggesting that it has the lowest valuation of the four firms. As we now demonstrate, calculating the percentile ranking ratios corrects this problem.

We first calculate the percentile ranking of market capitalization. We rank firms A, B, C, and D in order of their market capitalization, where the firm with the largest market cap is ranked first. In this case, that firm is IPO A. We then create the percentile ranking of market cap for each firm using the following common formula that indicates the percentage of values that fall *at or below* a particular value: *Percentile Rank =((B + 0.5E)/n)*100*, where *B* equals the number of firms with values ranked below firm *x*, *E* equals the number of firms with values of the same ranking as *x (*using this definition, *E* will always equal at least 1 because it is necessary to count firm *x* as we are calculating the percentage of values that fall *at or below* a particular value), and *n* equals the total number of firms. In this example, the percentile rank for IPO A's market cap would be 87.5%. The percentile rankings for firms B, C, and D are 62.5%, 37.5% and 12.5%, respectively. We then follow the exact same procedure to calculate the percentile ranking of earnings. Since IPO A has the lowest earnings, it has the lowest rank and hence, the lowest percentile ranking at 12.5%. Firms B, C, and D have earnings percentile rankings of 62.5%, 87.5%, and 37.5%, respectively. Finally, we form the ratio of the market cap and earnings percentile rankings for each firm. For IPO A that is: *Industry Percentile Ranking of Market Cap/Industry Percentile Ranking of Earnings= 87.5/12.5*=7. For firm B the ratio is 62.5/62.5=1, for firm C it is 37.5/87.5=0.43, and for firm D it is 12.5/37.5=0.34. Thus, we see that this method corrects the negative number problem and accurately assigns IPO A the highest valuation. The values for these ranking ratios will, of course, not be comparable to the original raw ratios, but will produce a reliable measure of relative valuation across firms that controls for industry effects.

Again, we use this method to create valuation ratios for each of the historical and contemporaneous accounting measures previously discussed. If the industry percentile ranking for EBITDA, sales, earnings, or book value falls below the first percentile, we round it up to the first percentile. As a result, the ratio of rankings is bounded between 0 and 100. Because the sample's ranking ratios tend to cluster, we use both the ratio and the natural log of the ratio in the analyses.

Whether or not our percentile ranking ratios accurately approximate relative valuation compared to the raw accounting multiples is important. In order to address this issue, we examine the correlations between our percentile ranking ratios and the raw multiples for *only* the subsamples where the raw multiples are meaningful, that is, where sales are non-zero and EBITDA, earnings and book values are positive. For example, the correlation between the percentile ranking ratio of price-to-sales and the raw price-to-sales multiple (where sales are positive) is 0.34191, significant at the 1% level; 726 of our 760 observations are used in the correlation. The correlations for all the percentile ranking ratios with the corresponding raw multiples are positive and significant at the 1% level. The remaining correlations are 0.27593 (price-to-EBITDA), 0.42377 (price-to-earnings), 0.75153 (market-to-book post offering), 0.40149 (enterprise value-to-EBITDA), and 0.25197 (enterprise value-to-sales). Thus, our percentile ranking ratio approach produces rankings that are relatively highly correlated with the meaningful raw multiples.

We argue that the percentile ranking approach is preferable to restricting the sample to firms where the raw ratios are meaningful because less information is lost in the former approach. Censoring the data based on the dependent variable could introduce serious bias in our results. The observations where the raw accounting multiples are inaccurate because of the negative/zero number issue are likely to be those firms with the highest valuations in the sample. A quick test of the data confirms this expectation. For example, if one ranks the firms in the sample based on the highest price-to-sales percentile ranking and then deletes those firms where the raw multiple is inaccurate because sales are zero (34 observations), we lose primarily the firms with the very highest price-to-sales percentile ranking; twenty-five of the observations dropped are among the 35 highest valuations and all 34 observations dropped fall among the highest 100 valuations. The price-to-sales multiple is the multiple where we would drop the fewest observations because the raw multiple is inaccurate, so the problem is exacerbated for those multiples where we would have to drop a higher fraction of the sample. If we follow the approach of dropping observations because the raw multiple is inaccurate and the observations dropped are those which would have otherwise had the highest relative valuation in the sample, we introduce a bias into the sample, which would likely artificially inflate negative effects on firm valuations because we are only observing those firms with relatively lower valuations in the sample. We consider this a serious issue and thus argue that the percentile ranking approach is the preferable method of dealing with the negative number problem.

A.3 Compliance Index

Similar to the approach in Chhaochharia and Grinstein (2007), the *Compliance Index* is formed by first creating a series of four dummy variables, each of which represents compliance with a particular aspect of the listing exchange's corporate governance listing requirements that followed the implementation of Reform. The four main governance restrictions the exchanges place on listing firms have to do with the composition of the board:

1. Firms must have a board composed of a majority of independent directors. The first dummy variable equals one if independent directors make up more than 50% of the board, and zero otherwise.
2. The audit committee must be composed of a minimum of three directors, and every member must meet not only the standard independence requirements of the exchange, but also the more stringent independence requirements of Rule 10A-3(b)(1) under the Securities Exchange Act of 1934.[14] The second dummy variable equals one if the audit committee meets both the minimum member and independence requirements, and zero otherwise.
3. The compensation committee must be composed entirely of independent directors. The third dummy variable equals one if the compensation committee meets the independence requirement, and zero otherwise.
4. The nominating committee must be composed entirely of independent directors. The fourth dummy variable equals one if the nominating committee meets the independence requirement, and zero otherwise.

[14] Specifically, the director "must not accept any consulting, advisory, or other compensatory fee from the company other than for board service, and they must not be an affiliated person of the company," as stated under Rule 5605-4 of the NASDAQ Equity Rules. The NYSE has the same listing requirement.

The *Compliance Index* is then the sum of these dummies, ranging in value from zero to four. Obviously, the more compliant the firm is with the independence requirements, the higher the *Compliance Index*.
Though one might assume that the *Compliance Index* equals 4 for all IPOs issued post-Reform, this is not the case. This is because we measure compliance *at the time of the offering* and all of the exchanges grant phase-in periods for compliance to companies listing at the time of their IPO. Specifically, companies must have one independent director on each of the audit, compensation and nominating committees at the time of the offering, a majority of independent directors on each committee within 90 days of listing, and fully independent committees within one year. Companies are allowed one year from the time of listing to meet the majority independent board requirement. This phase-in period is the same across the NYSE, Amex and NASDAQ exchanges.

A.4 Control Variables

A.4.1. Controlling for ownership structure

In assessing Reform's board structure provisions' effects on firm valuation, it is important to control for other sources of monitoring of firm behavior. Ownership structure is thought to be an alternative source of monitoring, both in general and in the IPO context (e.g. Brennan and Franks, 1997; Stoughton and Zechner, 1998). In addition, we are interested in the relative private benefits of control pre-Reform versus post-Reform and a common proxy for private benefits of control is ownership structure. Data on ownership structure is divided into the holdings of management, venture capitalists and private equity firms, all other institutional investors, and all other outside (non-employee) blockholders and directors. Management ownership (*MgtHold)* is defined as the percentage of equity retained by executive officers and employee-directors as a group. Note that this number does not include holdings by outside directors (such as venture capitalists with board seats). We further divide management ownership into the holdings of the CEO alone (*CEOHold)* and all other executive officers as a group (*OtherMgrHold*) to evaluate the relative importance of the ownership of each group. Venture capitalist holdings include the percentage of total venture capitalist (VC) and private equity fund blockholdings following the offering (*VCHold)*. Ljungqvist and Wilhelm (2003) consider any issuer backed by either a VC or private equity firm to be VC-backed. Venture capitalists and private equity funds are identified using annual volumes of Pratt's Guide to Venture Capital and Private Equity Sources.

Ownership by other institutional investors (*InstHold)* reflects the total blockholdings of non-venture capitalist institutions and corporations. Though their presence is not as prevalent as that of venture capitalists, occasionally both private and publicly traded firms will be among the pre-offering owners of an issuer. This variable does not include holdings of firms owned solely by executive officers of the issuer. Notes to the ownership section of the prospectus indicate whether a blockholding entity is owned by an executive officer of the issuer and the holdings of firms owned by executives are included in the shares beneficially owned by that executive. Directors and blockholders who are neither employees of the firm nor associated with a venture capitalist or any other institution are also occasionally among the pre-offering blockholders and their holdings are reflected in the *OtherOutsideHold* variable.

All ownership holdings are net of outstanding options and warrants exercisable within 60 days of the offering, which the SEC requires each firm to include in the shares beneficially owned by these individuals and entities. Options and warrants are excluded from the ownership holdings because we are interested in control rights at the offering and they may, and in some cases do, significantly artificially inflate pre-offering ownership that is diluted by the offering.

From prospectuses we also collect information on company founders and whether the firm has a dual-class common stock structure. To assess the role of founders, we code a series of dummy variables, *CEOFounder*, *ExecFounder, ChairFounder, DirFounder*, *BlockFounder,* each equaling one if the founder is the CEO, another executive officer, the chairman of the board, a director, or a non-employee, non-director blockholder, respectively.

In addition, we want to identify firms with dual class stock structures because when voting rights differ from cash-flow rights, Gompers, Ishii and Metrick (2010) show that, holding voting rights constant, an increase in insider cash-flow rights strengthens firm performance, but holding cash-flow rights constant, an increase in

insider voting rights weakens firm performance. This clearly has implications for firm valuations. A dual class share structure may also indicate that private benefits of control are higher. Thus, we create a dummy variable, *DualClass*, which equals one if the firm has a dual class stock structure, and zero otherwise.

A.4.2. Other control variables

We include a number of control variables in our empirical models that may, or have been shown by previous studies to, influence IPO valuations. These control variables are the natural log of offering proceeds, *Ln(Proceeds)*, the ratio of secondary share proceeds to total offering proceeds, *SecRatio*, firm age, *Age*, research and development and advertising expenditures for the last full fiscal year prior to the offering, *RDExp* and *AdvExp,* total assets before the offering, *Assets*, underwriter rank, *UWRank,* and a dummy variable equal to one if the firm lists on the Nasdaq and zero otherwise, *Nasdaq*.

The natural log of offering proceeds controls for the size of the offering and was found by Keloharju and Kulp (1996) to be significantly positively related to market valuations. The ratio of secondary share proceeds to total offering proceeds will likely negatively impact valuations because when the *SecRatio* is high, existing shareholders are selling a larger portion of their holdings and this sends a negative signal to investors about firm value per the Leland and Pyle (1977) signaling theory. Firm age is used in a number of valuation studies. Younger firms are more risky, but they often have higher market-to-book ratios than more mature firms because of their potential for growth.

We control for R&D because it is a proxy for growth opportunities (Smith and Watts, 1992; Gaver and Gaver, 1993; Baber, Janakiraman and Kang, 1996) and because it is also a measure of intangible assets which affect market valuations (Morck, Shleifer and Vishny, 1988; Lang and Stulz, 1994). Morck, Shleifer and Vishny (1988) also incorporate advertising expenditures as a measure of intangible assets. We also control for firm size using assets before the offering because firm value may be a function of firm size. Underwriter ranking is important to valuation because it can serve as a signal of firm quality (Booth and Smith, 1986; Hughes, 1986; Carter and Manaster, 1990; Ljungqvist and Wilhelm, 2003; Loughran and Ritter, 2004). We include the NASDAQ dummy variable because valuations could be sensitive to the listing exchange, perhaps because of liquidity differences (Ligon and Liu, 2011).

In addition to the other control variables, we collect information on the use of offering proceeds from prospectuses. If more than 50% of the offering proceeds are not assigned to a particular use (e.g. they are to be used for "general corporate purposes") or are to be used for operating expenditures, we code a dummy variable, *UseProcOpEx*, equal to one and zero otherwise. We are interested in the use of the offering proceeds because when management has broad discretion in the application of proceeds, the resulting uncertainty may affect valuations.

Appendix B: Robustness Checks

We address the possibility that the 2007-2008 financial crisis affects the results reported in Tables 6 and 7 in two different ways. First, we incorporate a financial crisis dummy, *Fin Crisis*, into the analyses reported in Tables 6 and 7 which is equal to one if the IPO took place between August 2007 and December 2008 and zero otherwise.[15] The results incorporating the *Fin Crisis* dummy into the Group 1 (analogous to the results in Table 6) and Group 2/3 (analogous to the results in Table 7) regressions are reported in Table B.1 and B.2, respectively. The Group 1 results are only slightly altered by the inclusion of the *Fin Crisis* dummy variable. The two changes that are notable are within the large-firm subsample; in the market-to-book value regression the *Compliance Index* loses significance and in the enterprise value-to-sales ratio regression, the *Post-Reform* dummy drops from significant at the 5% level to significant at the 10% level. There are no changes in the full sample results and the small-firm results are the same as those reported in Table 6.[16] The Group 2/3 results likewise change very little. The *Compliance Index* drops in significance from the 5% to the

[15] Alternatively, the financial crisis is defined as occurring between August 2007 and June 2009 and the results are virtually identical.

[16] The small-firm subsample results are identical to those reported in Table 6 because only one firm went public during the financial crisis in this subsample and that firm happened to be the only one within its two-digit SIC industry. Effectively, this analysis just replaced that industry dummy variable with the financial crisis dummy.

10% level in the enterprise value-to-sales regression for the full sample and the *Post-Reform* dummy loses significance in the enterprise value-to-sales regression for the large-firm subsample. All other significance levels are unaltered and coefficients on the variables change very little. Overall, what we see is that inclusion of the financial crisis dummy variable does not materially alter our results or conclusions.

Second, we exclude the IPOs that occurred during the financial crisis (between August 2007 and December 2008) as well as their pre-Reform counterparts and redo the analysis. Excluding the financial crisis IPOs and their pre-Reform matches from Group 1 eliminates 20 observations from the full sample, two of which come from the small-firm subsample and the remaining 18 of which come from the large-firm subsample. Compared to the results reported in Table 6, the main differences in the Group 1 results (reported in Table B.3) are that the *Compliance Index* becomes positive and significant at the 10% level in the price-to-earnings regression for full sample and small-firm subsample (where it had previously been insignificant) and it loses significance in the market-to-book value regression for the large-firm subsample (where it was previously negative and significant at the 10% level). There are no differences in the significance levels of the *Post-Reform* dummy variable.

The results excluding the financial crisis IPOs and their pre-Reform counterparts for Group 2/3 (analogous to the results reported in Table 7) are reported in Table B.4. More observations are dropped from Group 2/3 than Group 1; the full sample for Group 2/3 loses 38 observations, 4 of which come from the small-firm subsample and 34 of which come from the large-firm subsample. In the full sample, the *Compliance Index* increases in significance from the 5% to the 1% level in the price-to-sales regression, but loses significance in the market-to-book value regression. In the small-firm subsample, the variable loses significance in each of the price-to-earnings and extracted valuation factor regressions, where it had been negative and significant at the 5% level in the main analysis. In the large-firm subsample, the *Compliance Index* becomes positive and significant at the 10% level in the price-to-earnings regression, but negative and significant at the 10% level in the enterprise value-to-EBITDA regression and at the 5% level in the enterprise value-to-sales regression. In the full sample results, the *Post-Reform* variable loses significance in the market-to-book value regression, but becomes negative and significant at the 5% level in the enterprise value-to-sales regression. In the small-firm subsample results, it drops in significance from the 5% level to the 10% level in the price-to-earnings regression and from the 1% to the 5% level in the extracted valuation factor regression (both negative coefficients). Interestingly, it is not just the negative coefficients that drop in significance. The coefficients on the *Post-Reform* variable are positive, but less significant in the enterprise value-to-EBITDA regression (dropping from significance at the 1% level to the 5% level) and enterprise value-to-sales regression (dropping from significance at the 5% level to completely insignificant). Finally, in the large-firm subsample, the *Post-Reform* variable loses significance in the large-firm subsample.

Table B.1: Incorporating Financial Crisis Dummy Variable: Group 1
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to the financial crisis, ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is from the lowest tercile of the full post-Reform sample (i.e. the difference is 1.10 or less). Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 252 for the full sample, 40 for the small firm sub-sample, and 212 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	0.02	(0.48)	-0.06	(-1.06)	0.08	(1.33)	-0.06	(-1.32)	0.02	(0.41)	-0.07	(-1.18)	-0.01	(-0.19)
Small Firm	0.06	(0.49)	-0.53	(-3.14)***	0.15	(0.96)	0.27	(2.81)***	0.33	(0.76)	-0.27	(-0.58)	-0.48	(-5.75)***
Large Firm	0.01	(0.07)	-0.04	(-0.62)	0.04	(0.55)	-0.09	(-1.63)	0.01	(0.17)	-0.03	(-0.56)	-0.02	(-0.37)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.18	(-1.42)	0.06	(0.51)	-0.23	(-1.77)*	-1.02×10^{-3}	(-0.01)	-0.19	(-1.49)	0.04	(0.39)	0.02	(0.11)
Small Firm	-0.03	(-0.29)	0.29	(1.03)	-0.20	(-1.11)	0.03	(0.20)	0.31	(0.75)	0.63	(1.45)	0.06	(0.33)
Large Firm	-0.18	(-1.19)	-0.19	(-2.09)**	-0.23	(-1.40)	0.09	(0.62)	-0.17	(-1.09)	-0.17	(-1.94)*	-0.24	(-2.08)**

Table B.2: Incorporating Financial Crisis Dummy Variable: Group 2/3
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to the financial crisis, ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is in the highest tercile of the sample (i.e. the difference is 2.45 or more). Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 252 for the full sample, 70 for the small firm sub-sample, and 182 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.03	(-0.83)	-0.07	(-2.15)**	0.02	(0.32)	-0.06	(-2.01)**	-0.07	(-1.12)	-0.11	(-1.95)*	-0.03	(-0.88)
Small Firm	-0.07	(-0.94)	-0.14	(-0.94)	-0.16	(-2.05)**	-0.11	(-1.77)*	0.34	(1.46)	0.27	(1.02)	-0.23	(-2.22)**
Large Firm	-0.03	(-0.72)	-0.03	(-0.69)	0.06	(1.16)	-0.07	(-1.37)	-0.10	(-1.47)	-0.10	(-1.50)	0.02	(0.41)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.12	(-1.11)	-0.31	(-2.68)***	-0.18	(-1.28)	-0.17	(-2.01)**	-0.10	(-0.49)	-0.29	(-1.47)	-0.25	(-2.00)**
Small Firm	-0.33	(-1.15)	-0.74	(-2.15)**	-0.63	(-2.47)**	-0.54	(-2.20)**	2.22	(3.19)***	1.80	(2.31)**	-1.52	(-4.96)***
Large Firm	-0.16	(-1.13)	-0.15	(-1.08)	-0.10	(-0.56)	-0.21	(-1.72)*	-0.32	(-1.50)	-0.32	(-1.56)	-0.11	(-0.75)

Table B.3: Eliminating Financial Crisis IPOs: Group 1
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to the financial crisis, ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is from the lowest tercile of the full post-Reform sample (i.e. the difference is 1.10 or less), eliminating those IPOs that took place during the financial crisis (August 2007 to December 2008) and their pre-Reform matches. Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 232 for the full sample, 38 for the small firm sub-sample, and 194 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	0.05	(0.89)	-0.07	(-1.15)	0.10	(1.71)*	-0.06	(-1.26)	0.05	(0.85)	-0.07	(-1.22)	$-4.14x10^{-4}$	(-0.01)
Small Firm	0.04	(0.28)	-0.64	(-3.48)***	0.28	(1.72)*	0.38	(3.63)***	0.32	(0.72)	-0.35	(-0.75)	-0.50	(-4.66)***
Large Firm	0.04	(0.60)	-0.02	(-0.42)	0.06	(0.86)	-0.07	(-1.31)	0.05	(0.70)	-0.02	(-0.35)	0.01	(0.11)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.19	(-1.50)	0.06	(0.46)	-0.24	(-1.78)*	0.04	(0.33)	-0.20	(-1.55)	0.04	(0.36)	-0.09	(-0.85)
Small Firm	-0.10	(-0.83)	0.35	(1.16)	-0.09	(-0.55)	0.12	(0.64)	0.25	(0.56)	0.69	(1.36)	0.20	(0.95)
Large Firm	-0.21	(-1.44)	-0.21	(-2.29)**	-0.27	(-1.54)	0.17	(1.15)	-0.20	(-1.30)	-0.18	(-2.07)**	-0.25	(-2.13)**

Table B.4: Eliminating Financial Crisis IPOs: Group 2/3
Multivariate OLS Regression Analysis

This table presents results of the multivariate regression analysis of our valuation ranking measures on our test variables, Compliance Index and Post-Reform, as well as control variables related to the financial crisis, ownership structure, founder involvement, and other IPO characteristics for the subset of firms where the difference between the predicted and actual Compliance Index is in the highest tercile of the sample (i.e. the difference is 2.45 or more), eliminating those IPOs that took place during the financial crisis (August 2007 to December 2008) and their pre-Reform matches. Control variables related to ownership structure, founder involvement, and other IPO characteristics are included in each regression, but coefficients and t-statistics for the control variables are not reported to save space. With the exception of the extracted valuation factor, Valuation_F1, the results presented are those where the dependent variable is the natural logarithm of the original valuation ranking variables. All regressions are calculated using ordinary least squares and report White's (1980) heteroskedasticity-consistent standard errors. The results presented are for pre-offering price to EBITDA, price to sales, and price to earnings and post-offering market to book as the pre-/post- results are similar and these measures had better goodness of fit. For each dependent variable and test variable combination, the "Full" line presents the regression coefficient and the t-statistics for that test variable in the regression for each dependent variable, the line "Small Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having below $75 million in public float (which were exempt from SOX Section 404 during our data period), and the line "Large Firm" presents the regression coefficient and the t-statistics for the sub-sample of firms having above $75 million in public float (which were subject to SOX Section 404 during our data period). Sample sizes are 214 for the full sample, 66 for the small firm sub-sample, and 148 for the large firm sub-sample. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Compliance Index	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.05	(-1.43)	-0.10	(-2.71)***	$-2.06x10^{-5}$	(0.00)	-0.03	(-1.31)	-0.08	(-1.57)	-0.13	(-2.46)**	-0.05	(-1.46)
Small Firm	0.07	(0.65)	-0.13	(-1.34)	-0.05	(-0.68)	-0.11	(-1.94)*	0.23	(1.38)	0.03	(0.15)	-0.14	(-1.10)
Large Firm	-0.05	(-0.72)	-0.07	(-1.40)	0.12	(1.80)*	-0.01	(-0.25)	-0.11	(-1.79)*	-0.13	(-2.05)**	0.01	(0.26)
Post-Reform	Price/EBITDA		Price/Sales		Price/Earnings		M/B_{post}		EntVal/EBITDA		EntVal/Sales		Valuation_F1	
Sample	Coef.	T-stat.	Coef.	T-stat	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
Full	-0.13	(-1.29)	-0.37	(-3.23)***	-0.16	(-1.11)	-0.12	(-1.43)	-0.11	(-0.66)	-0.35	(-2.05)**	-0.28	(-2.39)**
Small Firm	0.06	(0.18)	-0.68	(-2.50)**	-0.41	(-1.65)*	-0.47	(-2.15)**	1.36	(2.44)**	0.61	(0.99)	-1.00	(-2.46)**
Large Firm	-0.11	(-0.76)	-0.23	(-1.52)	0.05	(0.24)	-0.08	(-0.65)	-0.21	(-1.08)	-0.34	(-1.68)*	-0.12	(-0.77)